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## Examining Moderators of the Relationship between Social Support and Self-Reported PTSD Symptoms: A Meta-Analysis

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Running Head: SOCIAL SUPPORT AND PTSD META-ANALYSIS

Examining Moderators of the Relationship between Social Support and Self-Reported PTSD

Symptoms: A Meta-Analysis

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### **Author Note**

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The data and coding manuals used in this meta-analysis have been deposited and can be viewed at: <https://doi.org/10.17605/OSF.IO/8TB7A>

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### Abstract

Social support is one of the most robust predictors of posttraumatic stress disorder (PTSD). Yet, little is known about factors that moderate the relationship between social support and PTSD symptom severity. This meta-analysis estimated the overall effect size of the relationship between self-reported social support and PTSD severity and tested meaningful demographic, social support, and trauma characteristics that may moderate this association using both cross-sectional and longitudinal effect sizes. A comprehensive search identified 139 studies with 145 independent cross-sectional effect sizes representing 62,803 individuals, and 37 studies with 38 independent longitudinal effect sizes representing 25,792 individuals. Study samples had to be comprised of trauma-exposed, non-clinical adult populations to be included in the analysis. Cross-sectional and longitudinal analyses revealed a near medium overall effect size ( $r_{\text{cross}} = -.27$ ; 95% CI:  $-.30, -.24$ ;  $r_{\text{long}} = -.25$ ; 95% CI:  $-.28, -.21$ ) with a high degree of heterogeneity (cross-sectional  $I^2 = 91.6$ , longitudinal  $I^2 = 86.5$ ). Both cross-sectional and longitudinal moderator analyses revealed that study samples exposed to natural disasters had a weaker effect size than samples exposed to other trauma types (e.g., combat, interpersonal violence), studies measuring negative social reactions had a larger effect size than studies assessing other types of social support, and veteran samples revealed larger effect sizes than civilian samples. Several other methodological and substantive moderators emerged that revealed a complex relationship between social support and PTSD severity. These findings have important clinical implications for the types of social support interventions that could mitigate PTSD severity.

**Keywords:** social support; posttraumatic stress disorder; meta-analysis; social negativity; veteran

### **Public Significance Statement**

This meta-analysis indicates that social support buffers against posttraumatic stress disorder (PTSD) symptoms among trauma-exposed individuals. The effect was weaker among individuals exposed to a natural disaster and stronger among veterans. The effect was also stronger when examining negative social reactions in response to trauma.

## Introduction

Posttraumatic stress disorder (PTSD) is a debilitating condition that is associated with significant chronic impairment (Bryant et al., 2016; Rodriguez, Holowka, & Marx, 2012; Solomon & Davidson, 1997) and increased risk for suicide (LeBouthillier, McMillan, Thibodeau, & Asmundson, 2015). In order to develop effective intervention strategies, it is important to understand key risk factors associated with the development, maintenance, and severity of PTSD. Social support has consistently been identified as one of the most robust predictors of PTSD (Brewin, Andrews, & Valentine, 2000; Ozer, Best, Lipsey, & Weiss, 2003). According to the stress-buffering model (Cohen & Willis, 1985), social support protects or buffers individuals from the pathogenic influence of trauma by enhancing individuals' perceived ability to cope with the trauma, reducing negative appraisals of the trauma, and reducing harmful physiological responses to the trauma. Thus, following trauma exposure, the expectation is that individuals with higher levels of social support will be less likely to develop PTSD symptoms. However, the stress-buffering model also stipulates that traumas demand specific coping responses and that there must be a match between the needs elicited by the trauma and function of support for buffering to occur. This is referred to as the matching hypothesis (Cohen & McKay, 1984; Cohen & Willis, 1985; Cutrona, 1990). Thus, the nature of the trauma, the nature of the support, and personal characteristics may moderate the relationship between social support and PTSD symptoms among individuals exposed to trauma.

Tests of the matching hypothesis are quite difficult to evaluate given that testing moderators requires large sample sizes, heterogenous populations, and a wide range of assessments. However, meta-analyses offer the opportunity to test moderators by exploring characteristics that account for variations in effect sizes between studies. To date, several meta-

analyses have examined social support and negative social reactions as predictors of PTSD symptoms and a PTSD diagnosis (Brewin et al., 2000; Dworkin, Brill, & Ullman, 2019; Ozer et al., 2003; Shand, Cowlshaw, Brooker, Burney, & Ricciardelli, 2015; Wright, Kelsall, Sim, Clarke & Creamer, 2013; Xue et al., 2015). However, each of these meta-analyses included a relatively small number of studies to evaluate the relationship between social support and PTSD (range: 4-33). Additional studies of social support and PTSD have been conducted since many of these meta-analyses were published, thus an updated synthesis of the literature is needed.

Dworkin and colleagues (2019) published the most recent meta-analysis on the topic; however, their meta-analysis focused exclusively on social reactions to disclosure of interpersonal violence, limiting the scope of the analyses in terms of both the type of social support and the nature of the trauma. Moreover, these meta-analyses all showed that the effect sizes of the relationship between social support and PTSD had significant heterogeneity; yet, to our knowledge, no existing meta-analyses have examined moderators of the relationship between social support and PTSD symptoms among a broad sample of trauma survivors. Based on the matching hypothesis of social support, we sought to replicate and extend previous meta-analytic findings by exploring key conditions, or moderators, under which social support is associated with PTSD among trauma-exposed individuals, including facets of social support, demographic characteristics, and trauma-related factors.

Social support is a multi-faceted construct that includes dimensions of perceived, enacted, and structural support, as well as negative social reactions (Barrera, 1986). *Perceived support* reflects a person's general beliefs about the availability of support and their satisfaction with support. *Enacted social support*, also known as tangible support, refers to actions that individuals take to aid another person. *Structural support*, also termed social embeddedness,



refers to the size and strength of an individual's support network. Finally, *negative social reactions* refer to behaviors from others that display negative affect (e.g., anger, dislike), display negative evaluation of the individual (e.g., criticism, blame), or create a hindrance to an individual's goals (Vinokur & van Ryn, 1993). Although the dimensions that underlie this construct are not particularly well-defined in the literature, the trauma literature has particularly focused on negative reactions in response to trauma disclosure (e.g., blaming the victim or treating the person differently after trauma; Ullman, 2000), social constraints that lead the survivor to feel unsupported or misunderstood (Lepore & Ituarte, 1999), and ways in which relationships may be sources of strain (e.g., making too many demands) or conflict (Butler, Koopman, Classen, & Spiegel, 1999).

There is evidence to suggest that different types of social support may differ in their relationship with PTSD severity. For example, several studies have shown that negative social reactions are more impactful than positive forms of support in predicting adjustment to trauma and trauma disclosure (Andrews, Brewin, & Rose, 2003; Davis, Brickman, & Baker, 1991; Ullman, 1996; Ullman & Peter-Hagene, 2016; Zoellner, Foa, & Brigidi, 1999). Two meta-analyses have explored how different types of positive and negative social exchanges relate to psychopathology. In their meta-analysis examining social reactions to disclosure of interpersonal violence, Dworkin and colleagues (2019) found that negative social reactions to trauma disclosure predicted more severe psychopathology. Positive social reactions to disclosure were not protective against psychopathology; however, perceiving others' reactions more positively was somewhat protective against psychopathology (Dworkin et al., 2019). In another meta-analysis, Finch and colleagues (1999) examined broad measures of social support (i.e., not specific to disclosure of trauma) and their relationship to psychological distress. They found that

negative social reactions revealed a moderate positive association with distress, and perceived support revealed a moderate negative association with distress, with no significant difference between the strength of the effect size for negative social reactions and perceived support. However, the effect size for perceived support was significantly larger than the effect sizes for enacted and structural support and the effect size for negative social reactions was significantly larger than the effect size for enacted support. These findings are consistent with the matching hypothesis. Specifically, S. Cohen proposes that perceptions of support that influence a person's ability to talk about their problems and how they feel about themselves would be a better buffer against stress compared to objective measures of support. To our knowledge, meta-analytic techniques have not been used to examine whether type of support affects the relationship between social support and PTSD severity.

In addition to the type of social support, the provider of social support may influence the effectiveness of social support as a buffer against PTSD. For example, support can come from a single individual (e.g., spouse), a social group (e.g., friends, military unit), or an authority figure (e.g., medical provider). Given that trauma occurs in specific contexts (e.g., combat deployment, medical settings, interpersonal trauma), it may be that specific providers of social support are particularly useful in buffering against PTSD. For example, DiMauro and colleagues (2016) observed that among military service members, higher perceived social support from family and friends was associated with lower PTSD symptom severity, but perceived support from the general public was unrelated to PTSD symptom severity. Similarly, Woodward and colleagues (2015) found that for survivors of intimate partner violence and motor vehicle accidents, support from friends and family predicted posttraumatic cognitions, which in turn predicted PTSD; however, support from a close other did not predict posttraumatic cognitions. These findings

suggest that social support source type may moderate the association of social support and PTSD symptom severity.

Several demographic characteristics such as sex, age, and race may also moderate the relationship between social support and PTSD. With respect to sex, gender role socialization encourages women to foster and maintain strong relationships (Levant, Richmond, Cook, House, & Aupont, 2007), with evidence suggesting that women are more likely to engage in a ‘tend and befriend’ response under conditions of stress (Taylor et al., 2000). Thus, poor social support in the context of trauma exposure may make women more vulnerable to distress than men. Among military service members, there is evidence that women perceive higher social support from family and friends compared to men, but this sex difference was nonsignificant when predicting PTSD symptom severity (DiMauro et al., 2016). Age may act as a moderator, given that social support exhibits a stronger negative association with mental health symptoms among younger individuals relative to older individuals (Milner, Krnjacki, & Lamontagne, 2016; Segrin, 2006; Weiner, Monin, Mota, & Pietrzak, 2016). Race may also play an important role; however, limited existing research examining race as a moderator of the relationship between social support and psychological distress has shown inconsistent results. For example, research suggests that among inner-city women in the United States, social support may have a stronger stress-buffering effect for Black individuals compared to White individuals (Gaffey et al., 2019). By contrast, among white-collar workers, Black women experience less of a stress buffering effect of social support compared to White women and men in general (Bailey, Wolfe, & Wolfe, 1996).

Sociocultural characteristics such as country of origin and veteran status may also moderate the relation between social support and PTSD. With respect to country of origin,

Western (and presumably individualistic) and non-Western (presumably collectivistic) cultures may have important differences that affect the degree to which social support buffers against PTSD. In general, individuals from collectivist cultures have been shown to have closer and more supportive networks than individuals from individualistic cultures (Triandis, Bontempo, Villareal, Asai, & Lucca, 1988). Thus, social support may be a particularly important means of buffering against PTSD symptoms within collectivist cultures. However, studies have also shown that compared with European Americans, Asians and Asian Americans are less willing to seek social support when dealing with stress (Taylor et al., 2004) and find social support to be less helpful when coping with stress (Kim, Sherman, Ko, & Taylor, 2006). Thus, those in non-Western cultures may have a weaker relationship between social support and PTSD. The relation between social support and PTSD may also differ between veterans and civilians given that veterans are often distanced from peer and institutional support when they return from deployment (Sherman, Larsen, & Borden, 2015). However, there is a relative dearth of research directly comparing the relationship between social support and PTSD based on veteran versus civilian status.

It is further possible that aspects of the trauma may affect the association between social support and PTSD, including the type of trauma exposure. Some traumas may confer such a strong risk for PTSD that social support may have minimal impact on PTSD severity. For example, interpersonal traumas are associated with a higher risk of PTSD than non-interpersonal traumas (Kessler, Sonnega, Bromet, Hughes, & Nelson, 1995), which could result in a weaker relationship between social support and PTSD following interpersonal traumas. Alternatively, it is possible that social support may be a critical antidote to interpersonal traumas, which are more likely to disrupt individuals' relationships and beliefs about others relative to non-interpersonal

traumas (Kern, Stacy, Kozina, Ripley, & Clapp, 2019). In this case, social support and PTSD would have a stronger relationship following interpersonal trauma. Notably, Woodward and colleagues (2015) showed that a model examining the relationship between social support and PTSD via posttraumatic cognitions did not differ between survivors of intimate partner violence (interpersonal trauma) and survivors of motor vehicle accidents (non-interpersonal trauma). It is also possible that certain traumas, such as natural disasters, are more likely to lead to tangible (e.g., shelter, food) rather than emotional needs; thus, general emotional support could have a weaker relationship with PTSD following such traumas based on the matching hypothesis.

In addition to the trauma type, the developmental timing of the trauma and the time since the traumatic event may affect the relationship between social support and PTSD. Similar to interpersonal trauma, child abuse is known to be particularly pathogenic (Ogle, Rubin, & Siegler, 2013). Moreover, those with childhood trauma are likely to experience a greater cumulative lifetime trauma load, which increases risk for PTSD (Kolassa et al., 2010). Thus, social support may have a weaker relationship with PTSD for individuals with childhood trauma exposure compared to individuals with trauma exposure in adulthood. With respect to the time since trauma exposure, one would expect that individuals' beliefs and symptoms become more fixed over time. Thus, the presence of social support soon after trauma may be more strongly associated with PTSD severity than social support many years after trauma. Understanding the trauma characteristics that affect the relationship between social support and PTSD will be critical for identifying the circumstances under which social support interventions may help to buffer against PTSD.

The purpose of the current meta-analysis was to assess the magnitude of the relationship between PTSD severity and social support and investigate possible moderators of this association

implicated by the matching hypothesis, including facets of social support and demographic and trauma characteristics. We examined cross-sectional effect sizes to maximize our potential to retrieve a sufficient number of studies to represent different categories of interest and test these moderators. We also examined longitudinal effect sizes to explore the direction of the relationship between social support and PTSD and moderators of this relationship. We focused on PTSD symptom severity as the outcome, rather than PTSD diagnosis, given that subthreshold PTSD is associated with significant impairment (Cukor, Wyka, Jayasinghe, & Difede, 2010). Based on the stress-buffering model, we hypothesized that there would be significant cross-sectional and longitudinal relationships between social support and PTSD symptoms. We further hypothesized that negative social reactions and perceived support would have a larger effect size than enacted and structural support based on previous research (Finch et al., 1999). All other moderator analyses were considered exploratory.

## **Method**

### **Search procedures**

Electronic databases were searched in three cycles to ensure adequate coverage of research outlets and search terms. In January 2014 and May 2017, PsycInfo, Embase + Medline, and PILOTS were searched using the following combination of terms: (social support OR instrumental support OR companionate support OR emotional support OR tangible support OR social connectedness OR criticism OR social constraint OR received support OR social integration OR functional support OR structural support OR informational support OR esteem support OR perceived support OR expressed emotion OR hostility OR social network OR cohesion OR social response OR social reaction OR disclosure OR social acknowledgment)

AND (PTSD or posttraumatic or post-traumatic). In June 2019, PsycInfo, PubMed<sup>1</sup> (includes Medline), PTSDPubs (formerly PILOTS), ProQuest Dissertations & Theses A&I, and ProQuest Dissertations & Theses Global were searched using the following combination of updated terms: (social support OR instrumental support OR companionate support OR emotional support OR tangible support OR social connectedness OR criticism OR social constraint OR received support OR social integration OR functional support OR structural support OR informational support OR esteem support OR perceived support OR expressed emotion OR hostility OR social network OR cohesion OR social response OR social reaction OR disclosure OR social acknowledgment OR enacted support OR social negativity OR social interaction\* OR network support) AND (PTSD or posttraumatic or post-traumatic). In this expanded search, a “not” limiter was included in the PsycInfo, PubMed + MEDLINE, and PTSDPubs searches to avoid redundant research reports already evaluated in the 2014 and 2017 searches. Each of these electronic database searches was restricted to reports available in English, and research conducted on adult human participants.

Four additional search strategies were also used to identify relevant research reports to be considered for inclusion in the meta-analysis. First, we reviewed the reference lists of relevant previous meta-analyses and systematic or other literature reviews along with all the references of journal articles that were deemed eligible for the meta-analysis. Second, journals that publish articles on PTSD were hand searched from 1980 or the journal’s first issue to June 2019 including *Journal of Traumatic Stress*, *Journal of Anxiety Disorders*, *Psychological Trauma: Theory, Research, Practice, and Policy*, and *Anxiety, Stress, and Coping*. Third, the corresponding author posted a request for unpublished data on several professional listservs

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<sup>1</sup> We switched from Embase to PubMed for the updated and expanded 2019 search because Embase was no longer available at Rush University Medical Center or the University of California, Irvine.

including the Association for Behavioral and Cognitive Therapies, the American Psychological Association Division of Trauma Psychology (Division 56), and the American Psychological Association Society for Military Psychology (Division 19). Fourth, all researchers who were the first, last, or corresponding author on at least two studies deemed to be eligible for the meta-analysis were emailed to request recently published data or unpublished data that might be eligible for the meta-analysis.

### **Inclusion Criteria**

The following criteria were applied to select studies for this meta-analysis. First, articles had to be full-text reports of a quantitative study written in the English language and published after 1980 when the diagnosis of PTSD was established (American Psychiatric Association, 1980). Second, articles had to include a sample in which all participants were exposed to a DSM-5 criterion A traumatic event (American Psychiatric Association, 2013). For military samples, we accepted articles of deployed combat veterans. Samples that examined family members or caregivers of loved ones who were suffering from trauma, illnesses, or died due to medical reasons were excluded due to difficulties in evaluating the degree of secondary trauma exposure. Third, articles had to report on participants that were 18 years of age or older, though the traumatic event may have occurred at any point throughout the lifespan. We selected this criterion because characteristics of posttraumatic stress disorder have been shown to be different among children compared to adults (American Psychiatric Association, 2013; Kaminer, Seedat, & Stein, 2005) and there are important differences in the social contexts of children and adults. Fourth, treatment studies were excluded and the study population could not be selected based on their PTSD symptoms or other psychiatric disorders. This criterion was included because individuals who are treatment seeking or who are selected based on their psychiatric symptoms



likely represent a biased sample of those who are traumatized. Moreover, these samples likely include individuals with a restricted range of PTSD symptoms, which could affect the correlation of PTSD with social support.

Fifth, articles had to include a well-validated self-report measure of PTSD severity that assessed re-experiencing, avoidance, and hyperarousal symptoms. We excluded measures designed to screen for PTSD but not designed to assess PTSD symptom severity, as well as measures of traumatic stress that captured symptoms beyond the scope of PTSD (e.g., the Trauma Symptom Checklist – 40). Sixth, PTSD symptoms had to be assessed at least one month after index trauma exposure (i.e., the trauma type that was the focus of the study) for all study participants. We used this criterion to be consistent with diagnostic distinctions between PTSD and acute stress disorder (American Psychiatric Association, 2013).

Finally, the article had to include a measure of social support that included a scale that went in a single direction from worse support to better support (i.e., scales with only a single dichotomous item and scales in which optimal support was in the center of the scale were excluded). We excluded studies of attachment, organizational support, support seeking, and family cohesion, as these were deemed to be separate constructs. If articles did not report the information needed to evaluate the inclusion/exclusion criteria, we requested it from the authors via email. If the information was not provided via email, we conservatively excluded the article.

### **Selection of studies**

Figure 1 displays the PRISMA flow diagram, which summarizes the study selection process and reasons for exclusion. A total of 8,270 records were identified through database searches and 490 records were identified through other sources. After removing duplicates, the titles and abstracts of 6,267 articles were inspected according to the inclusion and exclusion

criteria. Based on this review, 2,454 articles were identified as requiring a full-text review for inclusion. Nine of the titles / abstracts were of conference proceedings that could not be evaluated for inclusion and 31 articles could not be retrieved (dissertations or articles published in international journals that could not be retrieved through interlibrary loan or from the author / thesis chair). Therefore, 2,414 full-text articles were read and assessed for eligibility. Each article was read by two independent raters; in cases of disagreement, the two raters discussed and came to a consensus. Remaining questions regarding inclusion / exclusion were brought to the first author (initials masked for review) and the study team for discussion until a consensus was reached. If the article did not contain the necessary information to establish inclusion / exclusion, the corresponding author was contacted for clarification. If the author did not respond to the inquiry, the article was excluded.

All studies deemed to be eligible were then evaluated for sample overlap. We took a conservative approach in which studies that drew random samples from the same pool of participants were considered to be overlapping. In cases of overlapping samples, we selected the study with the largest available sample size. If the sample size was the same across several studies, we prioritized the study that was published first as it was likely to have the most methodological detail. If an effect size was not available and the author did not respond to our email inquiry for the effect size, then we went down the list of overlapping articles to identify any other studies with an available effect size.

### **Coding of studies**

A coding manual was developed by the first author (initials masked for review; public deposit link masked for review). Six psychologists who specialize in trauma-focused research and clinical work at an academic medical center received training in the coding manual. To

insure fidelity, the first author and all coders rated three sample articles independently and their ratings were then reviewed as a group to achieve consensus on the ratings. This procedure was repeated a total of four times (12 articles total) until independent fidelity was achieved. Coders were then placed into pairs, with each coder in the pair reviewing and rating the same set of articles. Ratings were compared between pair members, who met routinely to discuss and resolve discrepancies. Regular group meetings, including all coders and the first author, were used to address coding questions and maintain uniform decision making.

All eligible studies were coded for the following continuous characteristics: date of publication, mean age of participants, percent of female participants, and percent of White participants. Several dichotomous study quality and measurement characteristics were coded including whether the study was published in a peer-review journal, whether the PTSD measure was administered in English, whether the PTSD measure was rated based on a specific traumatic event, and whether the social support measure was validated. The PTSD measure used was coded; measures that were uncommonly used (used in < 5 studies) were collapsed into an “other” category. The Diagnostic and Statistical Manual (DSM) definition of the PTSD measure was captured as DSM-III, DSM-IV, or DSM-5.

The type of social support measured was coded as perceived support, enacted support, structural support, or negative social reactions. These designations were largely made based on construct definitions provided by the measure developers and / or study authors. In some cases where the measure was author-developed, coders used the item descriptions to code the measure. All coders were provided with the social support type definitions listed in the introduction. Any cases that were unclear were brought to the group for consensus. Notably, all measures were self-reported and therefore reflect an individuals’ perceptions of support in these different

domains. The provider of social support was coded as global / combination of sources, family, spouse, friends, troop / unit, medical provider, or other. For longitudinal effect sizes, we coded the timing of when the social support measure was assessed (before the trauma, during the trauma, or after the trauma) and the amount of time between the social support measure and PTSD measure (0 to <6 months, 6 month to <12 months, 1 year +, or unknown).

The sample population was coded as civilian, veteran, or both. Country of origin was coded and categorized as Western vs. non-Western based on the classification used in the International Epidemiological Association (IEA)'s series of reports on population health (Costantini et al., 2015). Accordingly, the category "Western" included any country in Western Europe as well as the USA and Canada. New Zealand was also categorized as "Western" because its historical ties as a dominion under the former British Empire largely continue to inform its systems of governance and economics as well as its legal and political institutions (Williams, 2018). All other countries were coded as non-Western, including countries in the following regions: Eastern Europe, Western Asia, East Asia, Southeast Asia, the Indian Subcontinent, and Sub-Saharan Africa.

The type of trauma was coded as combat or war, act of terror or mass violence, interpersonal violence, accident, natural disaster, medical illness, or mixed traumas. The developmental timing of trauma was coded as adulthood, childhood, mixed, or unknown. The time since trauma was coded as 1 month to < 6 months, 6 months to < 3 years, 3 years to < 10 years, 10 year +, or mixed / unknown. Time since trauma was coded based on the median, mean, mode, or range of time since trauma reported in the article (in that order of preference). If an article only presented a range that crossed two categories, the article was coded in the predominant category if the overlap was limited to one month, presuming that the central

tendency would fall within that category. If a range crossed two categories by more than a month, then the article was coded as mixed / unknown.

For cross-sectional effect sizes, four study quality items were coded that would potentially introduce bias: the internal reliability of the PTSD instrument  $> .7$  (Yes [1] vs. No / Not reported [0]); the internal reliability of the social support instrument  $> .7$  (Yes [1] vs. No / Not reported / single item measure [0]); the amount of score-level missing data  $< 20\%$  (Yes [1] vs. No / Not reported [0]); and if the authors used an appropriate method for handling missing data at the score level (scored 'yes' [1] if there was no missing data, if the authors used listwise deletion if there was less than 10% missing data, or if the authors used a multiple imputation procedure for more than 10% missing data). A measure of study quality for cross-sectional effect sizes was created by summing these four items. For longitudinal effect sizes, one additional quality item was coded: the percent of the sample that was retained at each longitudinal time point (80-100% retained [2], 50-79% retained [1], less than 50% retained or unknown [0]). A measure of study quality for longitudinal effect sizes was created by adding this item to the other four quality items.

For the effect size, we coded a bivariate correlation ( $r$ ) between a measure of PTSD severity and a measure of social support along with the sample size of that correlation. The magnitude of the correlation was interpreted as small (0.10), medium (0.30), or large (0.50; J. Cohen, 1992). If multiple measures of PTSD and/or social support were assessed in one study, all eligible effect sizes were coded. Effect sizes were coded such that higher levels of social support (lower levels of negative social reactions) represented higher scores and higher levels of PTSD represented higher scores. Therefore, the expected relationship between social support and PTSD was negative. If articles reported effect sizes in which poorer social support was

represented by higher scores, then the reported effect size was reversed. When an effect size was not available in the article, we contacted the study authors to request the data. A total of 158 studies did not report the necessary effect size for analysis; we received effect sizes for 37 of these studies. We also received effect size data for one unpublished study based on the request for data on professional listservs.

Both cross-sectional effect sizes and longitudinal effect sizes with social support preceding PTSD were coded. For cross-sectional effect sizes, if there were multiple time points in a study, then the first eligible time point (at least 30 days after trauma) in which both PTSD and social support was assessed was used to provide the largest sample size. For longitudinal effect sizes, all lags were coded using the social support measure assessed closest to the trauma and all subsequent eligible time points in which PTSD was assessed (at least 30 days after trauma). If social support was measured both before and after trauma, then all eligible lags were captured for both pre- and post-trauma administrations of social support.

### **Analyses**

Calculations of weighted effect sizes, heterogeneity, and moderators were conducted using Comprehensive Meta-Analysis version 3.3.070. Because considerable heterogeneity of effects was expected, random effects models were used to calculate the overall weighted effect size. For studies in which both total scores and subscale scores were reported for social support, only the total scores were included in the overall analysis. Heterogeneity of effect sizes was examined using the  $Q$  statistic and the  $I^2$  index. Specifically, the  $Q$  statistic was used to evaluate the significance of heterogeneity, whereas the  $I^2$  index was used to evaluate the proportion of variability in a set of effect sizes that is due to true between-study differences with percentages of 25, 50, and 75 representing low, medium, and high degrees of between-study variability,

respectively (Higgins, Thompson, Deeks, & Altman, 2003). To test for potential outliers, we conducted Grubbs' test using GraphPad (Grubbs, 1969). To evaluate the impact of publication bias, we created a funnel plot of the overall effect size (see supplement) and evaluated asymmetry of the funnel plot using Egger's test of the intercept (Egger, Smith, Schneider, & Minder, 1997) and Duval & Tweedie's trim-and-fill procedures (Duval & Tweedie, 2000). For Egger's test, when there is no evidence of asymmetry, the intercept is not significantly different from zero. The trim-and-fill method provides corrected effect sizes and confidence intervals that account for missing studies based on asymmetry of the funnel plot.

We then examined whether methodological characteristics were associated with both cross-sectional and longitudinal effect sizes to identify potential covariates for the substantive moderators of interest (i.e., sample, trauma, and social support characteristics). Mixed effect models were conducted using analysis of variance for categorical moderator variables and meta-regression analysis for continuous moderator variables. Any quality and measurement characteristics that were significantly associated with effect size at  $p < .05$  were examined as simultaneous predictors in a meta-regression to determine which variables were uniquely predictive of effect size. Those that remained significant in the meta-regression were included as covariates in subsequent analyses examining sample, trauma, and social support characteristics.

Finally, we examined sample characteristics, trauma characteristics, and social support characteristics as moderators of both cross-sectional and longitudinal effect sizes using analysis of variance for categorical moderator variables and meta-regression analysis for continuous moderator variables using mixed effects models. For several moderators (type of social support, provider of social support, timing of social support), there were instances in which different categories were nested within a single study (e.g., a single study measured different types of

social support). For these moderator analyses, we used the shifting unit-of-analysis approach (Cooper, 2010). For moderators that were significant, we then conducted meta-regression analyses including significant methodological characteristics as covariates. For categorical variables with more than two categories, if the omnibus test for the target moderator variable was significant at  $p < .05$  for cross-sectional analyses and  $p < .10$  for longitudinal analyses, we ran the meta-regression analyses with each category as the reference group (except the category with the smallest  $n_{\text{studies}}$ ) to conduct all pairwise contrast analyses.

## Results

### Cross-sectional Effect Sizes

**Descriptive Characteristics.** A total of 139 studies consisting of 145 unique samples were available for analysis (see Table S1 for study characteristics). Study sample sizes ranged from 22 to 10,734, resulting in a total of 62,803 individuals. The mean sample age was 39.9 ( $SD = 13.1$ ) and samples were 48.5% female and 68.4% White. Studies primarily originated from Western countries (81%), assessed civilians (69%), and assessed individuals with traumas that occurred in adulthood (83%). Trauma types included 33.8% combat / war, 15.2% medical illness, 13.8% natural disaster, 9.7% interpersonal violence, 7.6% acts of terror / mass violence, 4.1% accident, and 15.9% mixed / other. Among studies that reported time since trauma, 24.5% were in the 1 to <6 month range, 37.3% were in the 6 month to < 3 year range, 15.5% were in the 3 year to < 10 year range, and 22.7% were in the 10 year + range; however, the time since trauma could not be categorized for 24.1% of the total number of studies. A variety of self-report measures were used to assess PTSD severity, though versions of the PTSD Checklist (Blanchard, Jones-Alexander, Buckley, & Forneris, 1996; Weathers, Litz, Herman, Huska, & Keane, 1993; Weathers et al., 2013) were the most common (47.9%). Perceived social support was the most



commonly measured type of social support (70.1%), and measures of social support largely asked about global support (68.1%) rather than specifying the provider of social support. Authors developed their own measure of social support or used a single item to measure social support in 20.1% of studies.

**Overall Effect Size.** The overall random effects estimate was  $-.27$  (95% CI:  $-.30, -.24$ ,  $Z = -18.14$ ,  $p < .001$ ), indicating that higher levels of social support and lower levels of negative social reactions were associated with lower levels of PTSD severity (see Figure S1 for an effect size plot). No outliers were detected using Grubbs' test (Grubbs, 1969) and the estimates with one study removed ranged from  $-.275$  to  $-.265$ , suggesting that any potential outliers had minimal influence on the overall effect size. Heterogeneity analyses indicated a significant and high degree of heterogeneity in the estimate with over 90% of the total variance attributable to between-study variance ( $Q[df] = 1718.27(144)$ ,  $p < .001$ ,  $I^2 = 91.6$ ), suggesting that moderator analyses were appropriate. Egger's test of the intercept was significant ( $t(143) = 2.99$ ,  $p = .003$ ; see Figure S2 for the funnel plot). However, the trim-and-fill procedure using a random effects model indicated that no studies were missing to the right of the mean. Thus, the potential impact of publication bias was likely minimal, especially given the fact that asymmetry in the funnel plot may be attributable to heterogeneity rather than publication bias (Terrin, Schmid, Lau, & Olkin, 2003).

### **Moderator Analyses.**

**Methodological Characteristics.** We tested several moderators to evaluate whether aspects of the study quality and measurement characteristics were associated with the effect size. Table 1 presents the results of the categorical moderator analyses and Table 2 presents the results of continuous moderator analyses. Our quality measure, whether the data were published in a

peer-reviewed journal, and whether the effect size was reported in the article were not significant predictors of effect size. By contrast, year of publication was a significant predictor of effect size such that the effect size decreased over time (see Figure S3 for a scatterplot).

With respect to measurement characteristics, effect size was predicted by the PTSD measure used, the DSM definition of the PTSD measure, whether the PTSD measure was assessed based on a specific traumatic event, and whether the measure was administered in English. There was no difference in effect size between studies that used a validated measure of social support versus those that used author-developed measures / a single item to assess social support.

The five significant quality and measurement variables were included as simultaneous predictors in a meta-regression to determine which variables were unique predictors of effect size (see Table S2). In this analysis, publication date, the PTSD measure used, and whether the PTSD measure was assessed based on a specific event remained significant predictors of effect size. Therefore, these three variables were included as covariates in the substantive moderator analyses. Contrast analyses showed that studies using the Impact of Events Scale – Revised (IES-R; Weiss & Marmar, 1996) to assess PTSD severity had a weaker effect size compared to studies that used the PTSD Checklist (Blanchard et al., 1996; Weathers et al., 1993, Weathers et al., 2013), the Posttraumatic Diagnostic Scale / PTSD Symptom Scale – Self-report (Foa, Cashman, Jaycox & Perry, 1997; Foa Riggs, Dancu, & Rothbaum, 1993), or the Mississippi Scale for Combat-Related PTSD (Keane, Caddell, & Taylor, 1998; Norris & Perilla, 1996). Additionally, studies that used the Mississippi Scale for Combat-Related PTSD revealed a larger effect size compared to studies that used the Harvard Trauma Questionnaire (Mollica et al., 1992).

*Substantive Moderator Analyses.* Table 2 presents the results of continuous moderator analyses and Table 3 presents the results of categorical moderator analyses. For all substantive moderators, we first examined whether the moderator was associated with effect size. If the moderator was a significant predictor, we then conducted meta-regression analyses adjusting for the identified covariates (publication year, PTSD measure, PTSD assessed based on specific event).

*Sample Characteristics.* Results showed that country of origin and sample type (veteran vs. civilian) were significantly associated with effect size. However, after adjusting for covariates in meta-regression analyses, only country of origin remained a significant predictor of effect size (see Tables S3 and S4). Specifically, studies that came from Western countries had a larger effect size ( $r = -.30$ ) than studies that came from non-Western countries ( $r = -.12$ ). Sex, race, and age were not significant predictors of effect size.

*Trauma Characteristics.* Both the type of trauma and the developmental timing of the trauma were significant predictors of effect size. These predictors remained significant after adjusting for covariates in meta-regression analyses (see Tables S5 and S6). Specifically, studies that assessed individuals exposed to natural disaster had a much weaker effect size compared to all other trauma types. Notably, samples exposed to natural disaster had a non-significant weighted average effect size (95% CI included 0). With respect to the developmental timing of trauma, contrast analyses showed that studies in which individuals were exposed to trauma in adulthood had a significantly stronger effect size than studies with participants who had a mix of exposure in adulthood and childhood. The time since trauma exposure was not a significant predictor of effect size.

*Social Support Characteristics.* Type of social support was a significant predictor of effect size and remained significant after adjusting for covariates in meta-regression analyses (see Table S7). All four types of social support were significantly associated with effect size (95% CIs did not include 0) with studies assessing negative social reactions demonstrating the largest effect size ( $r = -.40$ ) followed by perceived support ( $r = -.27$ ), structural support ( $r = -.19$ ), and enacted support ( $r = -.15$ ). Contrast analyses showed that studies assessing negative social reactions had a significantly larger effect size than studies that assessed all other types of support. Additionally, studies that assessed perceived support had a significantly larger effect size than studies assessing enacted and structural support. The provider of social support was not a significant predictor of effect size.

### **Longitudinal Effect Sizes**

**Descriptive Characteristics.** A total of 37 studies consisting of 38 unique samples were available for analysis. Study sample sizes ranged from 21 to 10,807, resulting in a total of 25,792 individuals. The mean sample age was 39.2 ( $SD = 12.7$ ) and samples were 52.4% female and 68.8% White on average. Studies primarily originated from Western countries (89%), assessed civilians (79%), and assessed individuals with traumas that occurred in adulthood (92%). Trauma types included 26.3% combat / war, 15.8% medical illness, 15.8% interpersonal violence, 15.8% acts of terror / mass violence, 10.5% accident, 7.9% natural disaster, and 7.9% mixed / other. A variety of self-report measures were used to assess PTSD severity, though versions of the PTSD Checklist (Blanchard et al., 1996; Weathers et al., 2013) and the Posttraumatic Diagnostic Scale / PTSD Symptom Scale – Self-report (Foa et al., 1997; Foa et al., 1993) were the most popular (31.6% for each). Perceived social support was the most commonly measured type of social support (70.5%), and measures of social support largely asked about global support (77.5%)

rather than specifying the provider of social support. Social support was largely assessed after trauma exposure (89.7%). The lag between the social support and PTSD assessments was 0 to < 6 months for 32.5% of coded effect sizes, 6 months to < 12 months for 35.0% of effect sizes, and 12 months + for 32.5% of effect sizes. Authors developed their own measure of social support or used a single item to measure social support in 15.8% of studies.

**Overall Effect Size.** The overall random effects estimate was  $-.25$  (95% CI:  $-.28, -.21$ ,  $Z = -13.50$ ,  $p < .001$ ), indicating that higher levels of social support and lower levels of negative social reactions were associated with lower levels of PTSD severity (see Figure S4 for an effect size plot). Notably, the effect size observed in the longitudinal studies was nearly identical to the effect size observed in the cross-sectional studies. No outliers were detected using Grubbs' test (Grubbs, 1969) and the estimates with one study removed ranged from  $-.251$  to  $-.236$ , suggesting that any potential outliers had minimal influence on the overall effect size. Heterogeneity analyses indicated a significant and high degree of heterogeneity in the estimate with over 85% of the total variance attributable to between-study variance ( $Q[df] = 273.63(37)$ ,  $p < .001$ ,  $I^2 = 86.5$ ), suggesting that moderator analyses were appropriate. Egger's test of the intercept was significant ( $t(36) = 2.63$ ,  $p = .012$ ; see Figure S5 for the funnel plot). However, the trim-and-fill procedure using a random effects model indicated that no studies were missing to the right of the mean. Thus, the potential impact of publication bias was likely minimal, especially given the fact that asymmetry in the funnel plot may be attributable to heterogeneity rather than publication bias (Terrin et al., 2003).

#### **Moderator Analyses.**

**Methodological Characteristics.** We tested several moderators to evaluate whether methodological characteristics were associated with the effect size. Table 1 presents the results

of the categorical moderator analyses and Table 2 presents the results of continuous moderator analyses. Of the variables examined, there was only one significant predictor of the effect size: Validated social support measures revealed a larger effect size than author developed / single item measures. Therefore, this variable was included as a covariate in the substantive moderator analyses. Notably, the quality and measurement characteristics that predicted cross-sectional effect sizes (publication date, the PTSD measure used, whether PTSD was measured in English, and whether PTSD was rated to a specific event) were not replicated in analyses of longitudinal effect sizes.

***Substantive Moderator Analyses.*** Table 2 presents the results of continuous moderator analyses and Table 4 presents the results of categorical moderator analyses. For all substantive moderators, we first examined whether the moderator was associated with effect size. If the moderator was a significant predictor ( $p < .05$ ), we then conducted meta-regression analyses adjusting for the social support measure type (validated vs. author developed / single item).

***Sample Characteristics.*** Results showed that sample type (veteran vs. civilian) was significantly associated with the effect size, even after adjusting for covariates (see Table S8). Specifically, studies with veteran samples had a larger effect size ( $r = -.31$ ) than studies with civilian samples ( $r = -.22$ ). Sex, race, age, and country of origin were not significant predictors of effect size.

***Trauma Characteristics.*** Results showed that trauma type was a significantly predictor of effect size; after controlling for covariates, the omnibus test showed a trend ( $p = .056$ ), therefore we explored specific contrasts (see Table S9). Contrast analyses showed that studies assessing individuals exposed to natural disaster ( $r = -.13$ ) had a weaker effect sizes than studies assessing individuals exposed to combat / war ( $r = -.33$ ) and interpersonal violence ( $r = -.30$ ). Studies

assessing individuals exposed to medical illnesses ( $r = -.18$ ) also had a weaker effect size than studies assessing individuals exposed to combat / war. The time since trauma exposure was not a significant predictor of the effect size. We were not able to evaluate the developmental timing of trauma as a moderator because there was 1 study assessing individuals with trauma exposure in childhood, 1 study assessing individuals with a mix of trauma exposure in childhood and adulthood, and 1 study in which the timing of trauma exposure could not be coded.

*Social Support Characteristics.* Type of social support was a significant predictor of the effect size and remained significant after adjusting for covariates in meta-regression analyses (see Table S10). All three types of social support evaluated in this analysis were significantly associated with effect size (95% CIs did not include 0). Contrast analyses showed that studies assessing negative social reactions had a significantly larger effect size ( $r = .41$ ) than studies that assessed perceived ( $r = -.22$ ) and structural support ( $r = -.21$ ). The social support provider was a significant predictor of the effect size; however, this variable was no longer significant ( $p > .10$ ) after adjusting for covariates (see Table S11). The timing of social support was a significant predictor of the effect size such that effect sizes in which social support was measured after trauma ( $r = -.25$ ) were significantly larger than effect sizes in which social support was measured before trauma exposure ( $r = -.14$ ). This variable revealed a trend association ( $p = .087$ ) after adjusting for covariates (see Table S12).

## Discussion

In this meta-analysis, we examined the magnitude and moderators of the relationship between self-reported social support and PTSD severity among studies of trauma-exposed, non-clinical adult samples. The analysis included 148 cross-sectional effect sizes and 38 longitudinal effect sizes, representing a substantial increase in the number of included studies since previous

meta-analyses on this topic (Brewin et al., 2000; Ozer et al., 2003; Shand et al., 2015; Wright et al., 2013; Xue et al., 2015). The cross-sectional and longitudinal analyses revealed a very similar overall weighted effect size that was near medium in magnitude ( $r_{\text{cross}} = -.27$ ,  $r_{\text{long}} = -.25$ ), indicating that higher levels of social support and lower levels of negative social reactions were associated with lower PTSD symptom severity. These findings are consistent with previous meta-analyses examining the relationship between social support and PTSD symptoms / diagnosis (Brewin et al., 2000; Ozer et al., 2003; Shand et al., 2015; Wright et al., 2013; Xue et al., 2015). The longitudinal analyses, in particular, lend support to the stress-buffering model which identifies social support as an important protective factor against the development of posttraumatic stress symptoms after trauma exposure. Moreover, both cross-sectional and longitudinal analyses revealed that the effect size estimates had a high degree of heterogeneity. These findings are consistent with the premise of the matching hypothesis that different conditions (or moderators) will affect the degree to which the social support provided matches the needs elicited by the trauma, and therefore, the amount of buffering that will occur.

A key aim of the current meta-analysis was to evaluate how different types of social support (perceived support, enacted support, structural support, and negative social reactions) were associated with PTSD severity. Our findings showed that all four types of social support had significant weighted average effect sizes in both cross-sectional and longitudinal analyses, with the exception that we did not have a sufficient number of studies to evaluate enacted support in longitudinal analyses. Negative social reactions revealed the strongest effect size across both cross-sectional and longitudinal analyses; this effect size was significantly larger than the effect size for all other social support types, even after accounting for methodological covariates. Our findings with respect to the particularly deleterious effects of negative social



reactions are consistent with the results of Dworkin and colleagues' (2019) meta-analysis. However, it is notable that their meta-analysis indicated that positive social reactions to disclosure of interpersonal violence were not protective against psychopathology. By contrast, our results suggest that positive social support is protective against PTSD symptoms, but the harmful effects of negative social reactions on PTSD symptoms are more impactful than the salubrious effects of positive forms of social support. Notably, in our meta-analysis, many of the measures of negative social reactions evaluated social responses with respect to the trauma (e.g., negative responses to trauma disclosure) whereas positive social support measures were not typically trauma-specific. Given that Dworkin et al.'s (2019) meta-analysis focused specifically on social responses to disclosure, it is possible that broader forms of positive social support captured in our meta-analysis may be more protective against PTSD symptoms than positive responses to trauma disclosure. Additionally, the meta-analysis by Dworkin et al. (2019) suggests that there may be meaningful differences between perceived positive reactions to trauma disclosure (i.e., a person's subjective evaluation of support) and received positive reactions to trauma disclosure (i.e., observable supportive behaviors) in predicting psychopathology. This distinction between perceived and received support may also be confounded with measurement type such that perceived social support is typically captured by more global measures and received support is captured by trauma-specific measures. Further research is needed to disentangle the valence of social support (positive v. negative support), the context of support (general v. in response to disclosure), and the subjectivity of social support (perceived v. received). However, the toxicity of negative responses to trauma is clear.

Cross-sectional analyses further showed that the effect size for perceived support was significantly larger than the effect size for structural and enacted support; this difference between

perceived and structural support was not replicated in longitudinal analyses. Previous studies have revealed a bi-directional association between social support and PTSD symptoms (e.g., Shallcross, Arbisi, Polusny, Kramer, & Erbes, 2016; Ullman & Relyea, 2018). Thus, our cross-sectional findings may reflect the impact of PTSD symptoms on perceptions of support. More specifically, our findings may suggest that PTSD symptoms have a greater negative impact on perceived availability of and satisfaction with social support compared perceived structural and enacted support, consistent with findings by Platt and colleagues (2016). Though this was beyond the scope of the current study, future research exploring moderators of the longitudinal effects of PTSD severity on social support would help to address these questions.

Trauma type was a significant predictor of effect size in both cross-sectional and longitudinal analyses. Cross-sectional analyses indicated that the weighted effect size for studies that assessed natural disaster samples was non-significant and significantly smaller than the effect sizes for all other trauma types, even after adjusting for methodological covariates. Similarly, longitudinal analyses showed that studies assessing individuals exposed to natural disaster had a weaker effect size than studies assessing individuals exposed to combat / war and interpersonal violence. Based on the matching hypothesis, it is possible that natural disaster results in more financial or physical resource loss than other types of trauma and that perceived social support (which was assessed in over 70% of the effect sizes) is not particularly effective in buffering against these types of losses. It is also possible that other types of social support not captured in our study, such as community-level support, are more important in the context of natural disaster.

In both cross-sectional and longitudinal analyses, veteran samples revealed a larger effect size than civilian samples. This finding remained significant when adjusting for covariates in

longitudinal analyses, but not in cross-sectional analyses. Converging longitudinal analyses of trauma type showed that samples exposed to combat / war had a significantly larger effect size than samples exposure to natural disaster and medical illnesses (contrast  $ps < .05$ ) and a marginally larger effect size than samples exposed to acts of terror / mass violence (contrast  $p = 0.056$ ) and accidents (contrast  $p = 0.059$ ). Collectively, these findings suggest that having a supportive social system may be a particularly important buffer for veterans exposed to combat. Studies have shown that both unit support on deployment and post-deployment support are important predictors of post-deployment PTSD symptoms among veterans (Han et al., 2014, Pietrzak et al., 2010b; Wright, Kelsall, Sim, Clarke, & Creamer, 2013). Moreover, post-deployment support has been shown to be an important predictor of other important mental health outcomes including suicidality among trauma-exposed veterans (Kotler, Iancu, Efroni, & Amir, 2001; Jakupak, Vannoy, & Imel, 2010; Pietrzak, Russo, Ling, & Southwick, 2011, Pietrzak et al, 2009). Given the disruption of home-based social networks on combat deployment (Riggs & Riggs, 2011) and the disruption of unit-based social networks in the transition to home (Hinojosa & Hinojosa, 2011), these support systems may be especially vulnerable among deployed veterans. Thus, efforts to bolster support among combat veterans may be particularly valuable in mitigating PTSD symptoms and other important mental health outcomes.

In longitudinal analyses, the timing of when social support was assessed was a significant predictor of effect size. Specifically, studies assessing social support after trauma exposure had a stronger effect size than studies assessing social support prior to trauma exposure. This finding was only marginally significant after controlling for methodological covariates, which may be due to the fact that there were only four effect sizes assessing support prior to trauma exposure. Though preliminary, these findings suggest that one's experience of support in the aftermath of

trauma is particularly important in buffering against PTSD symptoms. This is quite logical given that the post-trauma period is when individuals are attempting to make meaning of the event and when coping demands are high.

Country of origin was a significant predictor of effect size in cross-sectional analyses such that studies from Western countries revealed a larger effect size than studies from non-Western countries. These findings remained significant after adjusting for covariates in cross-sectional analyses, but did not replicate in longitudinal analyses which only had four non-Western studies available for analysis. Thus, future research exploring the longitudinal relationship between social support and PTSD among non-Western cultures is clearly needed. There are a number of potential explanations for the cross-sectional findings that warrant future study. In general, individuals from collectivist cultures have been shown to have closer and more supportive networks (Triandis et al., 1988). Thus, it is possible that there may be a restriction of range among individuals in collectivist cultures, resulting in a smaller effect size. Another possibility is that the social support measures may not have the same construct validity across different cultures (Prince, 2008) or that the types of social support most important to buffering against PTSD in collectivist cultures (e.g., familism) are not adequately captured in these measures. There is also some evidence to suggest that individuals from more collectivist cultures are less willing to seek social support when dealing with stress (Taylor et al., 2004) and find social support to be less helpful in dealing with stress (Kim et al., 2006). In collectivist cultures with a more interdependent view of self, personal needs are seen as secondary to the group's needs (Markus & Kitayama, 1991). Thus, individuals from collectivist cultures who have experienced trauma may be less likely to share their traumatic experience so they can refrain from burdening others, avoid criticism, or maintain harmony (Chang, 2015; Kim et al., 2006;

Taylor et al., 2004). Notably, none of these cross-cultural studies are specific to trauma. Thus, further research is needed to understand how culture may impact beliefs about trauma disclosure, reactions to trauma disclosure, and social support seeking following trauma.

It is notable that a number of variables were not significant moderators of the relationship between social support and PTSD severity. Regarding sample characteristics, age, sex, and race were not significant predictors of effect size in either cross-sectional or longitudinal analyses. These findings suggest that the relationship between social support and PTSD severity is robust across individuals of different demographic makeups. Alternatively, the impact of demographic characteristics may be more nuanced, such as interactive effects between different demographic variables. Researchers working from an intersectionality framework have noted these effects can be difficult to capture in quantitative studies (Hinze, Lin, & Andersson, 2012). Time since trauma was also not a significant predictor of effect size, indicating that social support is not only relevant in the months after trauma exposure, but may also have an impact on PTSD severity long after trauma exposure. The social support provider was a significant predictor of longitudinal effect sizes; however, this finding did not hold after adjusting for covariates and was not found in cross-sectional analyses, suggesting that there were no consistent differences across the social support provider categories. These results suggest that individuals can experience meaningful support from a wide variety of sources. Collectively, these findings provide evidence that the relationship between social support and PTSD severity is robust across different types of individuals, different providers of support, and over time.

Moderator analyses evaluating the developmental timing of trauma revealed inconclusive findings that warrant further exploration in future research. Both non-adjusted and adjusted cross-sectional analyses indicated that studies assessing individuals with trauma exposure in

adulthood had a larger effect size than studies in which individuals had a mix of exposure in adulthood and childhood. Studies assessing individuals with trauma exposure only in childhood had an effect size that fell between those with adult and mixed exposure and this effect size was not significantly different from either group. We were not able to evaluate this moderator in longitudinal analyses because there was not a sufficient number of non-adult exposed samples. These findings are difficult to interpret, particularly given that we were not able to code studies for the degree of trauma load in the sample, which is associated with increased risk of PTSD (Kolassa et al., 2010). A more fine-grained examination is needed to understand how social support is associated with PTSD severity when trauma occurs during different developmental stages, including greater consideration of the types of social support that may be particularly relevant at different developmental phases and the role of important interpersonal factors that may impact engagement with social support and PTSD symptoms, such as attachment (Dieperink, Leskela, Thuras & Engdahl, 2001; Fraley, Fazzari, Bonanno, & Dekel, 2006; O'Connor & Elklit, 2008; Solomon, Dekel, & Mikulciner, 2008; Solomon, Ginzburg, Mikulciner, Neria & Ohry, 1998). Moreover, future research should attempt to disentangle the degree to which trauma timing and trauma load impact the relationship between social support and PTSD severity.

Our findings also point to important methodological issues in this literature. Notably, our quality measure did not predict the overall effect size in either cross-sectional or longitudinal analyses, thus studies were not weighted according to quality. However, the size of the cross-sectional effect appeared to diminish over time such that more recent publication dates were associated with smaller effect sizes. This finding may be due to an increased willingness to publish non-significant results and an increasing trend for authors to be more transparent in

reporting all outcome measures assessed. Publication date was not a significant predictor of the longitudinal effect size, suggesting that these concerns may not impact longitudinal studies. Cross-sectional analyses also showed that studies using the IES-R to assess PTSD severity had a non-significant average weighted effect size that was significantly smaller than studies that used other measures to assess PTSD. This pattern of results was similar in longitudinal analyses, but did not reach statistical significance. The IES-R differs from other PTSD measures in that it has more cognitive avoidance items than other measures (e.g., 7 items on the IES-R vs. 1 on the PCL), and it does not assess symptoms of anhedonia or feeling distant / cut off from others like other measures. These findings suggest that social support may be more strongly associated with mood and related symptoms of PTSD (including guilt and self-blame) compared to cognitive avoidance symptoms and that studies using the IES-R to evaluate the impact of social support should be interpreted with caution. Longitudinal effect size analyses also indicated that validated social support measures resulted in a larger effect size than author developed or single item measures, suggesting that validated measures should be preferred in future studies.

As noted above, our longitudinal results suggest that social support interventions may have a meaningful impact on PTSD severity. Given the robust relationship between social support and PTSD severity, surprisingly few interventions have targeted social support as a means of reducing PTSD severity. Cognitive-behavioral conjoint therapy for PTSD (CBCT for PTSD; Monson & Fredman, 2012) is a 15-session couples-based intervention designed to reduce PTSD and enhance intimate relationship functioning. One randomized controlled trial has been conducted to date and showed that CBCT for PTSD led to greater improvements in PTSD symptoms and relationship satisfaction relative to a waitlist control (Monson et al., 2012).

However, it remains unknown how CBCT for PTSD would perform relative to other evidence-based treatments for PTSD.

Two social support interventions have been developed that target reactions to trauma disclosure. Cordova and colleagues (2003) developed a two-session dyadic cognitive-behavioral intervention designed to facilitate trauma survivors' disclosure and increase supportive responses from a significant other. This intervention was then delivered by social workers and nurses at the emergency department (Des Groseilliers, Marchand, Cordova, Ruzek, & Brunet, 2013). Results showed that the treated group had significantly lower PTSD symptoms at two-year follow-up than those in the control group and none of the individuals in the treated group met criteria for PTSD at two-year follow-up. Edwards and Ullman (2018) also developed a two-hour group intervention to reduce negative reactions and increase supportive reactions to disclosure of sexual assault and intimate partner violence. The ultimate goal of this intervention was to reduce PTSD severity among college students who are at high risk of victimization by changing the nature of support they are likely to receive. A recent pilot randomized trial showed that individuals who received the intervention had a greater intention to provide positive social reactions compared to those in the waitlist control group 6-months after the intervention session (Edwards, Waterman, Ullman, et al., 2020). Moreover, exploratory analyses suggested that among the subset of participants that experienced unwanted sexual intercourse and/or physical intimate partner violence in the 6-month follow-up period, those who had received the intervention reported fewer PTSD symptoms than those in the control group (Edwards, Waterman, Dardis et al., 2020). However, it is notable that there were no differences in self-reported actual social reactions to disclosure between the intervention and the control group and almost two-thirds of those assigned to the treatment condition failed to receive the intervention,



raising concerns about the acceptability of the intervention (Edwards, Waterman, Ullman, et al., 2020). Additionally, the impact of this intervention on symptoms of trauma survivors who disclose to peers that receive the intervention has not yet been examined. These findings suggest that brief, economical social support interventions have promise in impacting PTSD symptoms, but more research is needed to establish acceptable and effective approaches. Our meta-analysis supports further development and evaluation of such interventions.

The current meta-analysis has a number of limitations that are important to consider when interpreting the results and considering future directions for research. Because the goal was to examine moderators of the relationship between social support and PTSD severity, we focused on studies using self-report measures rather than clinical measures, which are typically designed to diagnose the presence or absence of PTSD. Future research is needed to examine whether the current study findings are consistent for clinician-rated measures and whether the same moderators predict the absence versus presence of PTSD. Of the 110 articles excluded based on the criterion that PTSD had to be assessed at least 1 month after trauma exposure, 49 studies were excluded because it was clear or highly probable that participants in the study were trauma exposed within the past 30 days (e.g., in ongoing danger), 45 studies were excluded because time since trauma was unknown, and 16 studies represented overlapping samples. Our decision to exclude studies with unknown time since trauma may have preferentially excluded some trauma samples over others, though our assessment indicates that these 45 studies represented a wide range of traumatic events (interpersonal violence = 14, combat / war = 6, emergency personnel = 5, medical illness = 9, mixed trauma = 11). Additionally, our study excluded clinical samples due to concerns of a restriction of the range of PTSD symptoms in these populations. Although we expect that individuals within the non-clinical samples will meet threshold for PTSD, it is

possible that our results may not be generalizable to patient populations. Because we used the stress-buffering model as the framework for our approach, we did not evaluate the longitudinal relationship from PTSD to social support. Future research would benefit from a greater exploration into the ways that PTSD affects social support given evidence of the bi-directional relationship between social support and PTSD (e.g., Platt et al., 2016; Ullman & Peter-Hagene, 2016). Finally, it is important to note that despite identifying many significant moderators, there remained a significant amount of unexplained between-study variance. This suggests that other important moderators exist that we did not identify in this study.

Despite these limitations, this meta-analysis is the first to evaluate key moderators of the relationship between social support and PTSD symptoms and thus represents a critical advance in our understanding of the factors that affect this relationship. Our findings confirm robust cross-sectional and longitudinal relationships between social support and PTSD severity in a wide variety of populations exposed to varying trauma types. Moreover, our findings regarding the moderators of this relationship help to highlight with whom, when, and how these efforts might be most beneficial. Identifying strategies to reduce negative social reactions in response to trauma is a particularly important next step for future intervention research.

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Table 1.

## Moderator Analyses of Categorical Methodological Characteristics

Moderator	Cross-sectional effect sizes				Longitudinal effect sizes			
	$N_{\text{studies}}$	$r$	95% CI	$Q$ (df)	$N_{\text{studies}}$	$r$	95% CI	$Q$ (df)
Dissertation / unpublished data				0.02 (1)				0.01(1)
Yes	27	-.27	-.34, -.21		4	-.26	-.35, -.16	
No	118	-.27	-.30, -.24		34	-.24	-.28, -.21	
Effect size reported in article				0.70 (1)				0.66(1)
Yes	110	-.27	-.31, -.24		29	-.25	-.29, -.21	
No	35	-.25	-.29, -.21		9	-.22	-.28, -.17	
DSM definition used <sup>a</sup>				6.92(2)*				-
DSM-III	16	-.39	-.49, -.29		-	-	-	
DSM-IV	121	-.25	-.27, -.22		-	-	-	
DSM-5	8	-.27	-.33, -.20		-	-	-	
PTSD measure used				19.99 (5)**				5.37(4)
PCL	69	-.28	-.31, -.25		12	-.26	-.31, -.22	

IES-R	23	-.14	-.23, -.04	6	-.13	-.26, .01	
PDS / PSS-SR	23	-.27	-.34, -.21	12	-.22	-.27, -.18	
Mississippi	8	-.49	-.60, -.36	-	-	-	
HTQ	9	-.20	-.32, -.08	3	-.31	-.47, -.12	
Other	12	-.29	-.40, -.16	5	-.29	-.41, -.15	
PTSD rated to specific event							6.39 (1)*
Yes	50	-.22	-.26, -.19	15	-.21	-.25, -.18	1.63(1)
No	95	-.29	-.33, -.25	23	-.26	-.31, -.20	
PTSD measure in English							8.88 (1)**
Yes	99	-.30	-.33, -.27	25	-.22	-.24, -.19	1.95(1)
No	46	-.20	-.26, -.14	13	-.28	-.35, -.20	
Social support measure <sup>b</sup>							1.66 (1)
Validated	111	-.28	-.31, -.25	32	-.26	-.30, -.22	7.55(1)**
Author developed / single item	28	-.23	-.30, -.15	6	-.18	-.22, -.14	
Time between SS and PTSD <sup>c</sup>							1.45(2)
0 to <6 months	-	-	-	13	-.26	-.33, -.18	

6 months to <12 months	-	-	-	14	-.22	-.27, -.17
1 year +	-	-	-	13	-.26	-.32, -.21

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*Note:* PCL = PTSD Checklist, IES-R = Impact of Events Scale – Revised, PDS / PSS-SR = Posttraumatic Diagnostic Scale / PTSD

Symptom Scale – Self-Report, Mississippi = Scale for Combat-related PTSD, HTQ = Harvard Trauma Questionnaire.

<sup>a</sup>DSM definition was not examined as a moderator for longitudinal effect sizes because there were fewer than 3 studies in all categories other than DSM-IV.

<sup>b</sup>Studies were excluded from this analysis if they included both validated and author-developed / single item measures of social support.

<sup>c</sup>For moderators in which different categories were represented within a single study, we used a shifting the unit of analysis approach (Cooper, 2010).

Table 2.

## Meta-regressions of Continuous Moderators

Moderator	Cross-sectional effect sizes						Longitudinal effect sizes					
	N <sub>studies</sub>	Coef.	SE	Z	p	R <sup>2</sup> analog	N <sub>studies</sub>	Coef.	SE	Z	p	R <sup>2</sup> analog
Publication date	144	0.0105	0.0025	4.26	<.0001	0.19	38	0.0045	0.0028	1.64	0.1017	0.12
Study quality	138	-0.0160	0.0117	-1.36	0.1732	0.07	33	-0.0168	0.0132	-1.28	0.2021	0.00
Mean Age	127	-0.0019	0.0013	-1.42	0.1562	0.00	30	0.0018	0.0016	1.16	0.2452	0.00
% Female	136	0.0002	0.0005	0.40	0.6914	0.00	33	0.0007	0.0005	1.35	0.1778	0.00
% White	83	-0.0002	0.0008	-0.28	0.7832	0.00	22	-0.0007	0.0007	-0.98	0.3283	0.00

Table 3.

## Cross-sectional Moderator Analyses of Categorical Sample, Trauma, and Social Support Characteristics

Moderator	$N_{\text{studies}}$	$r$	95% CI	$Q_{\text{unadjusted}}$ (df)	Adjusted omnibus test
Country of Origin				21.03(1)***	$Z = -3.38$ ***
Western	117	-.30 <sup>a</sup>	-.33, -.27		
Non-Western	28	-.12 <sup>b</sup>	-.20, -.05		
Sample Type <sup>†</sup>				8.07(1)**	$Z = -0.80$
Civilian	98	-.23	-.26, -.20		
Veteran	44	-.32	-.37, -.27		
Type of trauma				30.66(6)***	$Q(\text{df}) = 20.32(6)$ **
Combat / war	49	-.32 <sup>a</sup>	-.37, -.26		
Acts of terror / mass violence	11	-.32 <sup>a</sup>	-.41, -.22		
Interpersonal violence	14	-.27 <sup>a</sup>	-.35, -.18		
Accident	6	-.24 <sup>a</sup>	-.49, .03		
Natural disaster	20	-.09 <sup>b</sup>	-.16, .02		

Medical illness	22	-.29 <sup>a</sup>	-.34, -.23		
Mixed / Other	23	-.27 <sup>a</sup>	-.31, -.23		
Timing of trauma <sup>†</sup>				10.00(2)**	$Q(df) = 8.29(2)^*$
Adulthood	110	-.29 <sup>a</sup>	-.32, -.26		
Childhood	11	-.20 <sup>a,b</sup>	-.28, -.11		
Mixed	11	-.13 <sup>b</sup>	-.25, -.00		
Time since trauma <sup>†</sup>				2.10(3)	-
1 month to < 6 months	27	-.24	-.31, -.18		
6 months to < 3 years	41	-.28	-.33, -.23		
3 years to < 10 years	17	-.28	-.35, -.21		
10 years +	25	-.32	-.39, -.24		
Social support type <sup>††</sup>				26.00(3)***	$Q(df) = 28.08(3)***$
Negative reactions	18	-.40 <sup>a</sup>	-.48, -.33		
Perceived	130	-.26 <sup>b</sup>	-.30, -.23		
Structural	24	-.19 <sup>c</sup>	-.24, -.14		
Enacted	12	-.15 <sup>c</sup>	-.24, -.07		

Social support provider <sup>††</sup>				11.79(6)	-
Global	124	-.29	-.32, -.26		
Family	15	-.21	-.28, -.14		
Spouse	10	-.21	-.31, -.10		
Friends	10	-.21	-.30, -.11		
Troop / unit	15	-.25	-.31, -.18		
Medical	3	-.15	-.41, .13		
Other	5	-.17	-.25, -.08		

*Note.* Adjusted analyses represent results for the overall significance of the moderator variable after adjusting for publication year, whether PTSD was rated to a specific event, and the PTSD measure. Different superscripts for the point estimates indicate that the average effect sizes are different at the  $p < .05$  level based on the adjusted meta-regression analyses. These contrast analyses were only conducted for moderators with a significant omnibus test.

<sup>†</sup>Studies with both civilians and veterans in the sample, studies with mixed / unknown trauma timing, and studies in which the time since trauma was unknown were excluded from the analysis.

<sup>††</sup>For moderators in which different categories were represented within a single study, we used a shifting the unit of analysis approach (Cooper, 2010).



Table 4.

## Longitudinal Moderator Analyses of Categorical Sample, Trauma, and Social Support

## Characteristics

Moderator	$N_{\text{studies}}$	$r$	95% CI	$Q_{\text{unadjusted}}$ (df)	Adjusted omnibus test
Country of Origin				0.07(1)	-
Western	34	-.25	-.28, -.21		
Non-Western	4	-.22	-.41, -.02		
Sample Type				4.75(1)*	$Z = -2.16^*$
Civilian	30	-.22	-.25, -.19		
Veteran	8	-.31	-.39, -.23		
Type of trauma				44.10(6)***	$Q(\text{df}) = 12.27(6)^\dagger$
Combat / war	10	-.33 <sup>a</sup>	-.39, -.26		
Acts of terror / mass violence	6	-.19 <sup>a,b</sup>	-.24, -.14		
Interpersonal violence	6	-.30 <sup>a,c</sup>	-.37, -.23		
Accident	4	-.19 <sup>a,b</sup>	-.31, -.06		
Natural disaster	3	-.13 <sup>b</sup>	-.15, -.11		
Medical illness	6	-.18 <sup>b,c</sup>	-.37, .02		
Mixed / Other	3	-.24 <sup>a,b</sup>	-.36, -.11		
Time since trauma <sup>†</sup>				0.03(2)	-
1 month to < 6 months	10	-.23	-.30, -.15		
6 months to < 3 years	22	-.22	-.26, -.18		

3 years +	6	-.23	-.33, -.13		
Social support type <sup>†</sup>				14.51(2)**	$Q(df) = 12.92(2)**$
Negative reactions	7	-.41 <sup>a</sup>	-.49, -.32		
Perceived	31	-.22 <sup>b</sup>	-.26, -.18		
Structural	6	-.21 <sup>b</sup>	-.31, -.12		
Social support provider <sup>i</sup>				23.61(2)***	$Q(df) = 4.56(2)$
Global	31	-.26	-.30, -.22		
Family / Spouse	5	-.15	-.17, -.14		
Friends	4	-.19	-.24, -.13		
Social support timing <sup>i, ii</sup>				13.99(1)***	$Z = 1.71^{\dagger}$
SS measured before trauma	4	-.14	-.18, -.09		
SS measured after trauma	35	-.25	-.28, -.21		

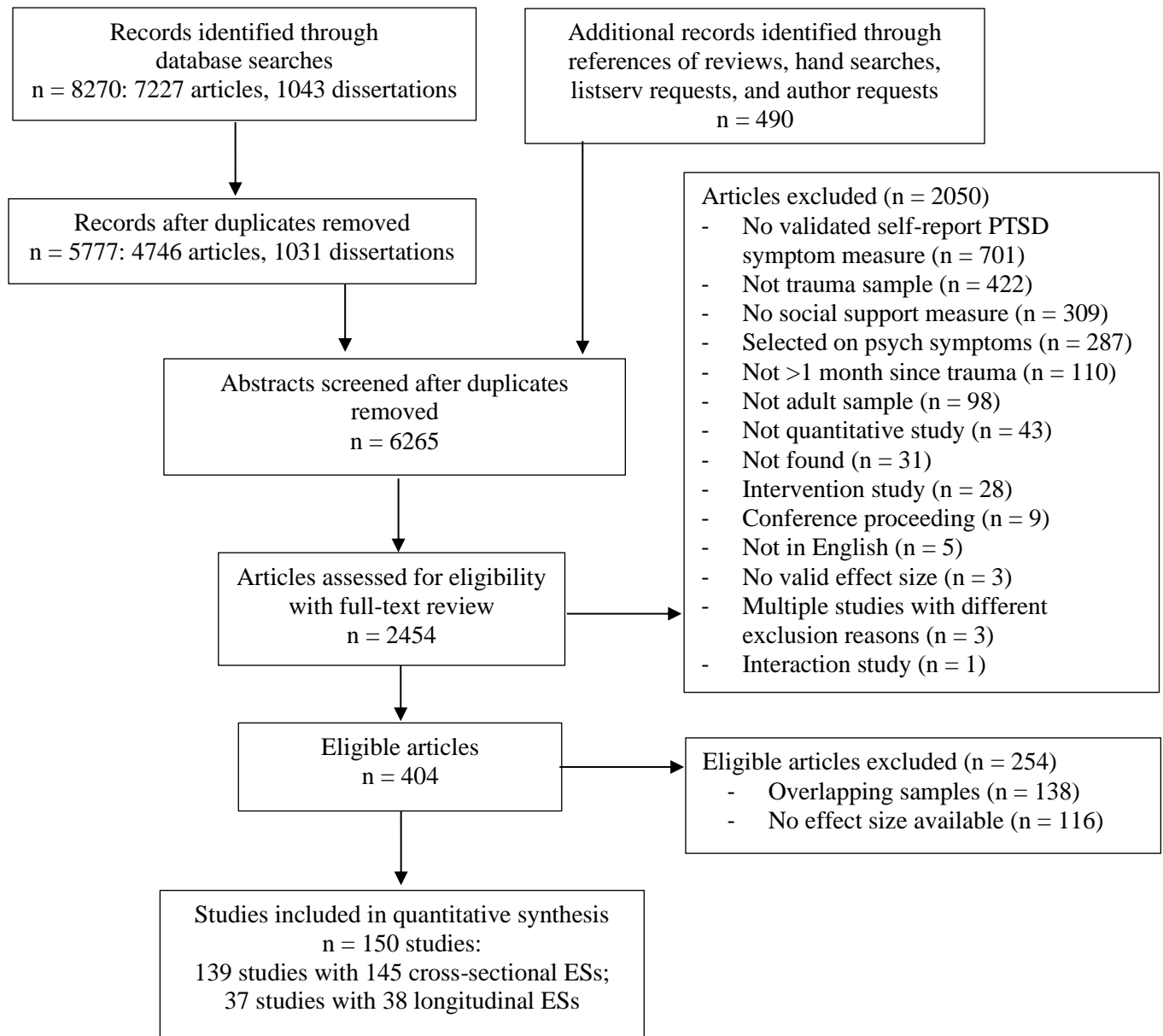
*Note.* Adjusted analyses represent results for the overall significance of the moderator variable after adjusting for whether the social support measure was validated. Different superscripts for the point estimates indicate that the average effect sizes are different at the  $p < .05$  level based on contrast analyses for the adjusted meta-regression analyses. These contrast analyses were only conducted for moderators with an omnibus test of  $p < .10$ .

<sup>†</sup> $p < .10$ , \* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .

<sup>i</sup>For moderators in which different categories were represented within a single study, we used a shifting the unit of analysis approach (Cooper, 2010).

<sup>ii</sup>Categories with less than 3 studies were excluded from the analysis.

Figure 1. PRISMA flow chart



*Note.* In the process of retrieving the full text of the reports from the database searches, several additional reports were identified (i.e., reports with very similar titles or additional reports sent to us by authors when reprints were requested). These reports were included in the total number of records identified through database searches. Of the 110 articles excluded based on the criterion that PTSD had to be assessed at least 1 month after trauma exposure, 49 studies were excluded because it was clear or highly probable that participants in the study were trauma exposed within the past 30 days (e.g., in ongoing danger), 45 studies were excluded because time since trauma was unknown, and 16 studies represented overlapping samples.